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THE RIDGEWAY, MILL HILL,
LONDON, N.W.7.

8th September, 1961.

Professor J. Lederberg,
Department of Genetics,
Stanford University Medical Center,
Palo Alto, California,
U. S. A.

Dear Josh,

Thank you very much for your letter of 28th August. By a mischance it arrived just after we went on vacation, and on returning I found a note from the editors saying that the episome paper would soon come out in page proof, so that I shall be very limited in the revisions I can make.

I wish I'd known about Thompson's intriguing early paper! The "common" properties of episomes are intended in a loose sense of the word, more as pointers than as definitions. Zinder's phage implies only the possibility of RNA episomes, and I will clarify this. The new work on P^{32} may be squeezed in briefly. It would be interesting to know more on F+ and F- metabolism, and I will write to Luca about this. The colicins I and E 1 interact in a complex way evidently, but in some circumstances the fertility is increased, so Ozeki & Howarth say, so in the short space it will have to be left rather vague.

I sincerely hope that no-one will think that the paper is intended to be a balanced, historical, or definitive review on episomes! Perhaps I should make this clearer at the beginning. It is, like most of the B.M.B. papers, a "current-trends-and-recent-work" thing, got up in somewhat of a hurry without very wide reading, and is in any event limited as to its length. So justice cannot be done to the known data or the development of the concepts. As it is already overlength I cannot expand it sufficiently to meet these needs.

Bill Hayes tells me that formaldehyde may reversibly inhibit chromosome transfer. I have not heard all the details, but gather that this occurs at concentrations well above that which we had in periodate tests. We had also checked that formaldehyde and glycerol-inactivated periodate were inactive at the appropriate concentrations. The slow recovery of maleness, and the inactivity of the postulated formaldehyde in mating mixtures of periodate-treated females makes it unlikely that formaldehyde could account for most of the periodate effect. It may be of interest to follow up, though, and I hope to get round to this soon. It might perhaps be relevant in the curious effect of periodate on DNA which you mention.

We have considered penicillinase genetics, but are still not happy with the enzymology of Bacillus subtilis enzyme, which is difficult in several respects. Martin Pollock has been away in Latin America for some time, as you may know, but before he left we discussed this and thought that some more enzymology was needed first.

At present I am swamped by editing with Ainsworth the next S.G.M. symposium on classification, so work is at a standstill for a while. Thank you for all the news, and our warm wishes to Esther and yourself.

Yours sincerely,

P.H.A. Sneath.